

Memo to: Oaktree Clients
From: Howard Marks
Re: Inspiration from the World of Sports

I'm constantly intrigued by the parallels between investing and sports. They're illuminating as well as fun, and thus they've prompted two past memos: "How the Game Should Be Played" (May 1995) and "What's Your Game Plan?" (September 2003). In the latter memo, I listed five ways in which investing is like sports:

- It's competitive – some succeed and some fail, and the distinction is clear.
- It's quantitative – you can see the results in black and white.
- It's a meritocracy – in the long term, the better returns go to the superior investors.
- It's team-oriented – an effective group can accomplish more than one person.
- It's satisfying and enjoyable – but much more so when you win.

Another angle on the investing/sports analogy has since occurred to me: an investment career can feel like a basketball or football game with an unlimited number of quarters. We may be nearing December 31 with a substantial year-to-date return or a big lead over our benchmarks or competitors, but when January 1 rolls around, we have to tackle another year. Our record isn't finalized until we leave the playing field for good. Or as Yogi Berra put it, "It ain't over till it's over." It was Yogi's passing in late September that inspired this memo. [Since most of the references in this memo are to American sports, with their peculiarities and unique terminology, this is a good time for an apology to anyone who's unfamiliar with them.]

Yogi Berra, Baseball Player

Lawrence "Yogi" Berra was a catcher on New York Yankees baseball teams for eighteen years, from 1946 to 1963. Although he was rarely number one in any offensive category, he often ranked among the top ten players in runs batted in, home runs, extra-base hits (doubles, triples and home runs), total bases gained and slugging percentage (total bases gained per at bat). He excelled even more on defense: in the 1950s he was regularly among the top three or four catchers in terms of putouts, assists, double plays turned, stolen bases allowed and base stealers thrown out.

Yogi was selected to play in the All-Star Game every year from 1948 through 1962. He was among the top three vote-getters for American League Most Valuable Player every year from 1950 through 1956, and he was chosen as MVP in three of those years. The Yankee teams on which he played won the American League pennant and thus represented the league in the World Series fourteen times, and they won the World Series ten times. He was an important part of one of the greatest dynasties in the history of sports.

To me, the thing that stands out most is Yogi's consistency. Not only did he perform well in so many different categories, but also:

- He led the American League in number of games played at the grueling catcher position eight years in a row.

- He was regularly among the catchers with the fewest passed balls and errors committed.
- He had around 450-650 at bats most years, but over his entire career he averaged only 24 strikeouts per year, and there was never one in which he struck out more than 38 times. (In 1950 he did so only 12 times in nearly 600 at bats.) Thus, ten times between 1948 and 1959 he was among the ten players with the fewest strikeouts per plate appearance.

In short, Yogi rarely messed up.

Consistency and minimization of error are two of the attributes that characterized Yogi's career, and they can also be key assets for superior investors. They aren't the only ways for investors to excel: some great ones strike out a lot but hit home runs in bunches the way Reggie Jackson did. Reggie – nicknamed "Mr. October" because of his frequent heroics in the World Series – was one of the top home run hitters of all time. But he also holds the record for the most career strikeouts, and his ratio of strikeouts to home runs was four times Yogi's: 4.61 versus 1.16. **Consistency and minimization of error have always ranked high among my priorities and Oaktree's, and they still do.**

Yogi Berra, Philosopher

Although Yogi was one of the all-time greats, his baseball achievements may be little-remembered by the current generation of fans, and few non-sports lovers are aware of them. He's probably far better known for the things he said:

- It's like *déjà vu* all over again.
- When you come to a fork in the road, take it.
- You can observe a lot by just watching.
- Always go to other people's funerals, otherwise they won't come to yours.
- I knew the record would stand until it was broken.
- The future ain't what it used to be.
- You wouldn't have won if we'd beaten you.
- I never said most of the things I said.

I've cited Yogi's statements in previous memos, and I borrowed the Yogi-ism at the top of the list above for the title of one in 2012. "Out of the mouths of babes," they say, comes great wisdom. The same was true for this uneducated baseball player, and many of Yogi's seeming illogicalities turn out to be profound upon more thorough examination.

"Baseball is ninety percent mental and the other half is physical." That was another of Yogi's dicta, and I think it's highly useful when thinking about investing. Ninety percent of the effort to outperform may consist of financial analysis, but you need to put another fifty percent into understanding human behavior. **The market is made up of people, and to beat it you have to know them as well as you do the thing you're considering investing in.**

I sometimes give a presentation called, "The Human Side of Investing." Its main message surrounds just that: while investing draws on knowledge of accounting, economics and finance, it also requires insight into psychology. Why? Because investors' objectivity and rationality rarely prevail as much as investment theory assumes, and emotion and "human nature" often take over instead. That's why my presentation is subtitled, "**In theory there's no difference between theory and practice. In practice there is.**" Yogi said that, too, and I think it's absolutely wonderful.

Things often fail to work the way investment theory says they should. Markets are supposed to be efficient, with no underpricings to find or overpricings to avoid, making it impossible to outperform. But exceptions arise all the time, and they're usually attributable more to human failings than to math mistakes or overlooked data.

And that leads me to one of the most thought-provoking Yogi-isms, concerning his choice of restaurant: "Nobody goes there anymore because it's too crowded." What could be more nonsensical? If nobody goes there, how can it be crowded? And if it's crowded, how can you say nobody goes there?

But as I wrote last month in "It's Not Easy," a lot of accepted investment wisdom makes similarly little sense. And perhaps the greatest – and most injurious – of all is the near-unanimous enthusiasm that's behind most bubbles.

"Everyone knows it's a great buy," they say. That, too, makes no sense. **If everyone believes it's a bargain, how can it not have been bought up by the crowd and had its price lifted to non-bargain status as a result?** You and I know the things all investors find desirable are unlikely to represent good investment opportunities. But aren't most bubbles driven by the belief that they do?

- In 1968, everyone knew the Nifty Fifty stocks of the best companies in America represented compelling value, even after their p/e ratios had reached 80 or 90. That belief kept them there . . . for a while.
- In 2000, everyone thought tech investing was infallible and tech stocks could only rise. And they were sure the Internet would change the world and the stocks of Internet companies were good buys at any price. That's what took the TMT boom to its zenith.
- And here in 2015, everyone knows social media companies will own the future. But will their valuations turn out to be warranted?

Logically speaking, the bargains that everyone has come to believe in can't still be bargains . . . but that doesn't stop people from falling in love with them nevertheless. Yogi was right in indirectly highlighting the illogicality of "common knowledge." **As long as people's reactions to things fail to be reasonable and measured, the spoils will go to those who are able to recognize this contradiction.**

Looking for Lance Dunbar

There may be a few folks in America who, like the rest of the world's population, are unaware of the growing popularity of daily fantasy football. In this on-line game, contestants assemble imaginary football teams staffed by real professional players. When that week's actual football games are played, the participants receive "fantasy points" based on their players' real-world accomplishments, and the participants with the most points win cash prizes. (Why is it okay to engage in interstate betting on fantasy football but not on football itself? Because proponents were able to convince the authorities that the act of picking a team for fantasy football qualifies it as a game of skill, not chance. But last week, Nevada became the sixth state to ban daily fantasy sports, concluding that it's really nothing but gambling.) **The commercials for fantasy football say things like, "Sign up, make your picks, and collect your winnings."** That sounds awfully easy . . . and not that different from discount brokers' ads during bull markets.

In daily fantasy football, the challenge comes from the fact that the participants have a limited amount of money to spend and want to acquire the best possible team for it. **If all players were priced the same**

regardless of their ability (a completely inefficient market), the prize would go to the participant who's most able to identify talented players. And if all players were priced precisely in line with their ability (a completely efficient market), it would be impossible to acquire a more talented team for the same budget, so winning would hinge on random developments.

The market for players in fantasy football appears to be less than completely efficient. Thus participants have the possibility of finding mispricings. A star may be overpriced, so that he produces few fantasy points per dollar spent on him. And a journeyman might be underpriced, able to produce more rushing (i.e., running) yards, catches, tackles or touchdowns than are reflected by his price. That's where the parallel to investing comes in.

Smart fantasy football participants understand that the goal isn't to acquire the best players, or players with the lowest absolute price tags, but players whose "salaries" underestimate their merit – those who are underpriced relative to their potential and might amass more points in the next game than the cost to draft them reflects. Likewise, smart investors know the goal isn't to find the best companies, or stocks with the lowest absolute dollar prices or p/e ratios, but the ones whose potential isn't fully reflected in their price. In both of these competitive arenas, the prize goes to those who see value others miss.

There's another similarity. Sports media employ "experts" to cover this imaginary football league, and it's their job to attract viewers and readers by offering advice on which players to draft. (What other talking heads does that remind you of?) My musings on fantasy football started in late September, when I heard a TV commentator urge that participants take a look at Lance Dunbar, a running back for the Dallas Cowboys, based on the belief that Dunbar's price might underestimate his potential to earn fantasy points. The commentator's thesis was that the Cowboys' star quarterback was injured and, because of the replacement quarterback's playing style, Dunbar might get more opportunities – and run up more yardage – than his price implied. Thus, Dunbar might represent an underappreciated investment opportunity.

Or not. Dunbar tore his anterior cruciate ligament in the next game, meaning he won't produce any more points – real or virtual – this season. It just proves that even if your judgment is sound, randomness has a lot of influence on outcomes. You never know which way the ball will bounce.

"Sign up, make your picks, and collect your winnings." If only everyone – fantasy football entrants and investors alike – understood it's not that easy.

Are the Helpers Any Help?

In investing, there are a lot of people who'll offer to enhance your results . . . for a fee. In an allegorical treatment in Berkshire Hathaway's 2005 annual report, Warren Buffett called them "Helpers." There are helpers in sports, too, especially where there's betting. This memo gives me a chance to discuss an invaluable clipping on the subject that I collected nine months ago and have been looking for an occasion to mention.

The *New York Post*'s sports writers opine weekly as to which professional football games readers should bet on (real games, not fantasy). Each week, the *Post* reports on the results of the prognostications for the season to date. When they published the results last December 28, they might have thought they demonstrated the value of those helpers. But I think that tabulation – nearly at the end of the football season – showed something very different.

By the time December 28 had rolled around, the eleven forecasters had tried to predict the winner of each of the 237 games that had been played to date, as well as what they thought were their 47 or so “best bets.” By “the winner,” I assume they meant the team that would win net of the bookies’ “point spread.” (Without doing something to even the odds, it would be too easy for bettors to win by backing the favorites. To make betting more of a challenge, the bookies establish a spread for each game: the number of points by which the favored team has to beat the underdog in order to be deemed the winner for betting purposes.) How often were the *Post*’s picks correct? Here’s the answer:

<u>Percentage correct</u>	<u>Total picks (2,607 games)</u>	<u>Best bets (522 games)</u>
All forecasters	50.9%	49.4%
Median forecaster	50.6	47.9
Best forecaster	58.5	56.2
Worst forecaster	44.8	39.6

An incorrigible optimist – or perhaps the *Post* – might say these results show what a good job the forecasters did as a group, since some were right more often than they were wrong. But that’s not the important thing. For me, the key conclusions are these:

- The average results certainly make it seem that picking football winners (net of the points spread) is just a 50/50 proposition. Evidently, the folks who establish the point spreads are pretty good at their job, so that it’s hard to know which team will win.
- The symmetrical distribution of the results and the way they cluster around 50% tell me there isn’t much skill in predicting football winners (or, if it exists, these pickers don’t have it). The small deviations from 50% – both positive and negative – suggest that picking winning football teams for betting purposes may be little more than a matter of tossing a coin.
- Even the best forecasters weren’t right much more than half the time. While I’m not a statistician, I doubt the fact that a few people were right on 56-58% of their picks rather than 50% proves it was skill rather than luck. Going back to the coin, if you flipped one 47 times (or even 237 times), you might occasionally get 58% heads.
- **Lastly, all eleven writers collectively – and seven of them individually – had worse results on the games they considered their “best bets” than on the rest of the games.** So clearly they aren’t able to accurately assess the validity of their own forecasts.

And remember, these forecasts weren’t made by members of the general populace, but rather by people who make their living following and writing about sports.

My favorite quotation on the subject of forecasts comes from John Kenneth Galbraith: “We have two classes of forecasters: Those who don’t know – and those who don’t know they don’t know.” Clearly these forecasters don’t know. But do they know it? And do their readers?

The bottom line on picking football winners seems to be that the average forecaster is right half the time, with exceptions that are relatively few in number, insignificant in degree and possibly the result of luck. He might as well flip a coin. **And that brings us back to investing, since I find this analogous to the**

observation that the average investor's return equals the market average. He, too, might as well flip a coin . . . or invest in an index fund.

And by the way, the average participant's average result – in both fields – is before transaction costs and fees. **After costs, the average investor's return is below that of the market. In that same vein, after costs the average football bettor doesn't break even.**

What costs? In sports betting, we're not talking about management fees or brokerage commissions, but "vigorish" or "the vig." Wikipedia says it's "also known as *juice*, the *cut* or the *take* . . . the amount charged by a bookmaker . . . for taking a bet from a gambler." This obscure term refers to the fact that to try to win \$10 from a bookie, you have to put up \$11. You're paid \$10 if you win, but you're out \$11 if you lose. N.b.: bookies and sports betting parlors aren't in business to provide a public service.

If you bet against a friend and win half the time, you end up even. But if you bet against a bookie or a betting parlor and win half the time, on average you lose 10% of the amount wagered on every other bet. So at \$10 per game, a bettor following the Post's football helpers through December 28, 2014 would have won \$13,280 on the 1,328 correct picks but lost \$14,069 on the 1,279 losers. Overall, he would have lost \$789 even though slightly more than half the picks were right. **That's what happens when you play in a game where the costs are high and the edge is insufficient or non-existent.**

Another Look at Performance Assessment

This memo gives me an opportunity to touch on another recent sporting event: Super Bowl XLIX, which was played last February. I'm returning to a subject I covered at length in the "What's Real?" section in "Pigweed" (February 2006), which was about the meltdown of a hedge fund called Amaranth. Among the ways I tried to parse the events surrounding Amaranth was through an analogy to the Rose Bowl game played at the end of the 2005 college football season to determine the national champion. In the game, the University of Texas beat the favored University of Southern California. While leading by five points with less than three minutes left to play, USC had a fourth down with two yards to go for a first down. They lost largely because – in something other than the obvious choice – the coach elected to go for it rather than punt the ball away, and they were stopped a yard short. UT got the ball and went on to score the winning touchdown. Before the game, USC had widely been considered one of the greatest teams in college football history. Afterwards there was no more talk along those lines. Its loss hinged on that one very controversial play . . . controversial primarily because it was unsuccessful. (Had USC made the two yards and earned a first down, they would have retained the ball and been able to run out the clock, sealing a victory.)

Something very similar happened in this year's Super Bowl. The Seattle Seahawks were trailing the New England Patriots by a few points. On second down, with just 26 seconds to go and one timeout remaining, the Seahawks had the ball on the Patriots' one-yard line. Everyone was sure they would try a run by Marshawn Lynch (who in the regular season had ranked first in the league in rushing touchdowns and fourth in rushing yards), and that he would score the winning touchdown. But the Seahawks' maverick coach, Pete Carroll – ironically, also the coach of USC's losing Rose Bowl team – tried a pass play instead. The Patriots intercepted the pass, and the Seahawks' dreams of a championship ended.

"What an idiot Carroll is," the fans screamed. "Everyone knows that when you throw a pass, only three things can happen (it's caught, it's dropped or it's intercepted) and two of them are bad." The Seahawks lost a game they seemed to be on the verge of winning, and Carroll was vilified for being too bold and wrong . . . again. **His decision was unsuccessful. But was it wrong?**

With assistance from Warren Min in Oaktree's Real Estate Department, I want to point out some of the considerations that Carroll may have taken into account in making his decision:

- Up to that point in the season, more than 100 passes had been attempted from the one-yard line, and none of them had been intercepted. So Carroll undoubtedly expected that, at the very worst, the pass would be incomplete and the clock would stop (as it does after incomplete passes) with just a few seconds elapsed. That would have given the Seahawks time for one or two more plays.
- With only 26 seconds remaining and the Seahawks down to their last timeout, if they ran and Lynch was stopped, the clock would have kept running (as it does after rushing plays). Seattle would then have been forced to either use their precious timeout or try a hurried play.
- Malcolm Butler, the defender who intercepted Seattle's pass, was a rookie playing in the biggest game of his life, and he was undersized relative to Ricardo Lockette, the wide receiver to whom the pass was thrown.
- According to *The Boston Globe*, of Lynch's 281 carries during the 2014 regular season, 20 had resulted in lost yardage and two more had yielded fumbles. In other words, the Seahawks had experienced a setback 7.8% of the time when Lynch carried the ball. Further, Lynch had been handed the ball at the one-yard line five times in 2014, but he scored only once, for a success rate of 20%. Thus it was no sure thing that Lynch would be able to gain that needed yard against a defense expecting him to run.

To the first-level thinker, Carroll's decision to pass looks like a clear mistake. Maybe that's because great running backs seem so dependable, or because passing generally seems like an uncertain proposition. Or maybe it's just because the pass was picked off and the game lost: outcomes strongly bias perceptions.

The second-level thinker sees that the obvious call – to run – was far from sure to work, and that doing the less-than-obvious – passing – might put the element of surprise on the Seahawks' side and represent better clock management. Carroll made his decision and it was unsuccessful. But that doesn't prove he was wrong.

Here's what my colleague Warren wrote me:

The media and “talking heads” completely buried the decision to throw because of one data point: the pass was intercepted and the Seahawks lost the game. But I don’t believe this was a bad decision. In fact, I think this was a very well-informed decision that more people possessing all the data might have made given ample time to analyze the situation.

As you always say, you can't judge the quality of a decision based on results. If we somehow were able to replay this game in alternative realities to test the results, I think the Seahawks' decision wouldn't look so bad. But they certainly lost, perhaps because of bad luck. Now, similar to the USC/Texas situation, the media has written some very significant storylines regarding legacies:

- The Patriots secured “dynasty” status by winning four Super Bowls since 2001.
- Tom Brady, the Patriots’ quarterback, is hailed as one of the greatest of all time.
- The Seahawks’ defense, which was talked about as being “the greatest ever,” is lauded no more (despite the fact that it wasn’t defense that lost the game).

But should this one victory – which swung on a single play – really place the Patriots and Tom Brady among the greatest? And was Carroll actually wrong? All of this goes back to one of my favorite themes from *Fooled by Randomness* by Nassim Nicholas Taleb, for me the bible on how to understand performance in an uncertain world.

In his book, Taleb talks about “alternative histories,” which I describe as “the other things that reasonably could have happened but didn’t.” Sure, the Seahawks lost the game. But they could have won, and Carroll’s decision would have made the difference in that case, too, making him the hero instead of the goat. **So rather than judge a decision solely on the basis of the outcome, you have to consider (a) the quality of the process that led to the decision, (b) the *a priori* probability that the decision would work (which is very different from the question of whether it did work), (c) the other decisions that could have been made, (d) all of the events that reasonably could have unfolded, and thus (e) which of the decisions had the highest probability of success.**

Here’s the bottom line:

- There are many subtle but logical reasons for arguing that Coach Carroll’s decision made sense.
- The decision would have been considered a stroke of genius if it had been successful.
- Especially because of the role of luck, the correctness of a decision cannot necessarily be judged from the outcome.
- You clearly cannot assess someone’s competence on the basis of a single trial.

What all the above really illustrates is the difference between superficial observation and deep, nuanced analysis. **The fact that something worked doesn’t mean it was the result of a correct decision, and the fact that something failed doesn’t mean the decision was wrong. This is at least as true in investing as it is in sports.**

The Victor’s Mindset

It often seems that just as I’m completing a memo, a final inspiration pops up. This past weekend, the *Financial Times* carried an interesting interview with Novak Djokovic, the number one tennis player in the world today. What caught my eye was what he said about the winner’s mental state:

I believe that half of any victory in a tennis match is in place before you step on the court. **If you don’t have that self-belief, then fear takes over.** And then it will get too much for you to handle. It’s a fine line. (Emphasis added)

Djokovic’s statement reminded me of a conversation I had earlier this month, on a subject I’ve written about rarely if ever: self-confidence. It ranks high among the attributes that must be present if one is to achieve superior results.

To be above average, an athlete has to separate from the pack. To win at high-level tennis, a player has to hit “winners” – shots his opponents can’t return. They’re hit so hard, so close to the lines or so low over the net that they have the potential to end up as “unforced errors.” In the absence of skill, they’re unlikely to be executed successfully, meaning it’s unwise to try them. But people who possess the requisite skill are right in attempting them in order to “play the winner’s game” (see “What’s Your Game Plan”).

These may be analogous to investment actions that Yale’s David Swensen would describe as “uncomfortably idiosyncratic.” The truth is, most great investments begin in discomfort – or, perhaps

better said, they involve doing things with which most people are uncomfortable. To achieve great performance you have to believe in value that isn't apparent to everyone else (or else it would already be reflected in the price); buy things that others think are risky and uncertain; and buy them in amounts large enough that if they don't work out they can lead to embarrassment. What are examples of actions that require self-confidence?

- Buying something at \$50 and continuing to hold it – or maybe even buying more – when the price falls to \$25 and “the market” is telling you you’re wrong.
- After you’ve bought something at \$50 (thinking it’s worth \$200), refusing to “prudently take some chips off the table” when it gets to \$100.
- Going against conventional wisdom and daring to “catch a falling knife” when a company defaults and the price of its debt plummets.
- Buying much more of something you like than it represents in the index you’re measured against, or entirely excluding an index component you dislike.

In each of these cases, the first-level thinker does that which is conventional and easy – and which doesn’t require much self-confidence. The second-level thinker views things differently and, as a consequence, is willing to take actions like those described above. **But they’re unlikely to be done in the absence of conviction.** The great investors I know are confident second-level thinkers and entirely comfortable diverging from the herd.

It’s great for investors to have self-confidence, and it’s great that it permits them to behave boldly, **but only when that self-confidence is warranted. This final qualification means that investors must engage in brutally candid self-assessment.** Hubris or over-confidence is far more dangerous than a shortage of confidence and a resultant unwillingness to act boldly. That must be what Mark Twain had in mind when he said, “It ain’t what you don’t know that gets you into trouble. It’s what you know for sure that just ain’t so.” **And it also has to be what Novak Djokovic meant when he said, “It’s a fine line.”**

So there you have some of the key lessons from sports:

- For most participants, success is likely to lie more dependably in discipline, consistency and minimization of error, rather than in bold strokes – high batting average and an absence of strikeouts, not the occasional, sensational home run.
- But in order to be superior, a player has to do something different from others and has to have an appropriate level of confidence that he can succeed at it. Without conviction he won’t be able to act boldly and survive bouts of uncertainty and the inevitable slump.
- Because of the significant role played by randomness, a small sample of results is far from sure to be indicative of talent or decision-making ability.
- The goal for bettors is to see value in assets that others haven’t yet recognized and that isn’t reflected in prices.
- At first glance it seems effort and “common sense” will lead to success, but these often prove to be unavailing.
- In particular, it turns out that most people can’t see future outcomes much better than anyone else, but few are aware of this limitation.
- Before a would-be participant enters any game, he should assess his chances of winning and whether they justify the price to play.

These lessons can serve investors very well.

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